powers which, according to the doctrine of evolution, they do not acquire, but inherit. For the benefit of those who would now save this theory by maintaining that it meant or means something that was never intended, we would quote the example given in illustration by M. Ribot:—"Few of our necessities are so pressing as thirst; nevertheless an animal does not distinguish at first that the water in the pond can appease it; it is only later in his wanderings that he comes to apply his tongue to the surface of the water (happy accident) and to feel the relief which it affords, and thus to learn what he ought to will." Few of the poor animals, we fear, would ever reach maturity if they had not more of instinct than Prof. Bain would here allow them. Yet what Prof. Bain has written about instinct he claims, and M. Ribot thinks "justly, as one of the most original portions of his work." Unfortunately for the fame of this celebrated psychologist, it appears from the progress of research that exactly in those departments where he has been most original have his conceptions been least in accordance with the order of Nature.

M. Ribot's most serious labour seems to have been in bringing together, in a more or less connected form, the psychology which has hitherto been scattered through the writings of Mr. George Henry Lewes. This original thinker and highly suggestive writer is the only one of our psychologists whose work may not be regarded as finished. The volume recently published ("Problems of Life and Mind") does not supply material for an estimate of the work on which he has long been engaged. But while continuing to agree with Mr. Spencer much more than any other of the authorities, Mr. Lewes encourages his readers to hope for important and permanent additions to mental philosophy; and to put the prospects of the work at the lowest, he will certainly compel the school to which he belongs to gravely reconsider some of their fundamental positions.

When in his conclusion M. Ribot attempts to bring forward the points on which the writers are agreed, the "fundamental propositions" to which he reduces them are unsatisfactory in two ways. Many of them are so vague in expression as not to exclude rival theories; while others have a sufficient amount of precision to make them flat contradictions of the clearly expressed and reiterated opinions of some of the authorities. We are, for example, not surprised to hear a disciple of Mr. Mill and Prof. Bain express his astonishment that his masters should have fathered on them the realism they have so vigorously opposed. M. Ribot's words are explicit:—"Outside of us, and independently of our perceptions, there exists a material world which condemns idealism. It is conformable to the data of the sciences to believe that this material world, taken in itself, does not resemble the perceptions of it which we have; this condemns vulgar realism." It surely says little for idealism that M. Ribot, after studying and expounding the arguments in its favour, should thus end with making our idealists agree with that very realism which Prof. Bain has described as unworthy the name of Philosophy.

After recognising the shortcomings referred to, it remains to be repeated that the author deserves the thanks of everyone interested in the spread of mental science in France. But we are unable to find any reason for the book having been translated into English. No English student ought to go to M. Ribot for the opinions of Mr. Mill or Mr. Spencer. Should any not already familiar with the topics discussed attempt to read the work, they will frequently be much perplexed by the exceeding carelessness of the translation. If they are amused to read that "*melodies* are described in pathological treatises," they may be a little puzzled to make out how "all Science is *contradicted* by the double action of analysis and synthesis," or in what sense "so long as the living being has no consciousness he leads a purely psychological life." And we would hint to any innocent young persons disposed to pin their faith to Locke, that they may be in some danger of being misunderstood should they follow the uniform usage of the translator and describe themselves as "*sensualists*."

DOUGLAS A. SPALDING

OUR BOOK SHELF

Africa: Geographical Exploration and Christian Enterprise. By A. Gruar Forbes. (London: Sampson Low and Co. 1874.)

WE can recommend this moderate-sized volume as an interesting popular *résumé* of the progress of discovery in Africa from the earliest time to the present day. The author writes mainly from the point of view of missionary enterprise, but seems to have read with diligence and intelligence the greater part of the literature of modern African travel, with which his book is mostly concerned, and has made therefrom a creditable compilation showing the progress of discovery from Bruce downwards. The first chapter gives a brief account of the topography, climate, and productions of Africa; and the accompanying pretty clear map shows the route of the leading explorers. We notice one or two signs of carelessness or haste; for example, on p. 4, Mr. Forbes states that "the most westerly point is Cabo Verde, in long. 51° 25′ E., lat. 10° 25′ N., the distance between the two points being about the same as its length." Again, at p. 115, "Sahara Desert" ought surely to be "Kalahari Desert."

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Ocean Circulation—Dr. Carpenter and Mr. Croll

IN the interests of Science, of scientific discussions, and of scientific men let me be allowed to protest very earnestly against the manner in which Dr. Carpenter has thought fit to reply in your columns to the defence which Mr. Croll made against the representation of his views, given in NATURE, vol. ix. p. 423. I take much interest in the subject under discussion—the great fundamental cause of the distribution of heat over the globe, and am most anxious to arrive at the true solution of the problem—a result, however, which will be indefinitely postponed if such letters as that of Dr. Carpenter in NATURE, vol. x. p. 62, are to become common.

Mr. Croll, discarding unimportant details, asked attention to one or two cardinal "misapprehensions" on which Dr. Carpenter had been proceeding. But the Doctor, instead of plainly grappling with these alleged "misapprehensions," runs off to call attention to a footnote of another paper of Mr. Croll's, brings forward some statement of Mr. Croll's views about the relative saltness of different portions of the ocean (about which, however, not a single word is said in the letter that has called forth Dr. Carpenter's reply), and concludes by another *argumentum ad hominem*, of which I am sure every reader of his papers must now be weary.

Now I strongly object to have dust thrown in my eyes in this way. Dr. Carpenter does not attempt to deal with any one of the cardinal and crucial arguments in Mr. Croll's letter. He raises a cloud about "averages," repeats his joke about ten children to every marriage, and with other irrelevant matter, including an introduction of the Astronomer Royal

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and Prof. Mohn, suddenly disappears. Not, however, without adding a sentence which I am sure he will in the end regret. He says he has "been forced by the personal attacks in which Mr. Croll has latterly thought fit thus to indulge to retort upon him." Why, the discovery of anything "personal" in Mr. Croll's writings would be as great a find as the true theory of oceanic circulation. I do not know any papers in our contemporary scientific literature more thoroughly undeserving of such a charge. Surely a man may call in question, nay, may even take a little quiet fun out of another man's opinions or crotchets without laying himself open to the stigma of being guilty of "personal attacks." Besides, it seems to me that Dr. Carpenter's charge is inappropriate. Mr. Croll, remarking "with some reluctance" that he was "compelled to refer" to Dr. Carpenter's continual quotation of eminent physicists who had adopted his views, while none had shared in the objections to them, merely assured Dr. Carpenter that such was not the case, and made reference to one person as an illustration, but without giving the person's name. The Doctor, as everybody knows, has been profuse in his use of this kind of argument. And now the moment it is used against himself, he denounces the introduction of "personal attacks!"

I purposely avoid entering into the merits of the question. What, in common with every sincere well-wisher of Science, I desire to see, is its thorough, honest and courteous discussion. Dr. Carpenter's high position gives a weight to what he says and does, which adds much to the regret with which his letter will be perused. That this protest may be received on its own merits and without reference to the pen which holds it, I withhold my name.

F. R. S.

Proportionality of Cause and Effect

MR. HAYWARD now affects the air of an injured man, and complains of being charged with "confusing issues" which he neither "raised nor accepted." He may be convicted out of his own mouth. The following passage occurs in his last letter but one (NATURE, vol. x. p. 25):—"It should be noted that my principal 'exemplification of *unconsciously-formed preconceptions*' was of Mr. Spencer's own choosing, namely, Newton's 'Second Law of Motion.'" In his last he says:—"The object of my remarks was simply to test the truth of a definite assertion of Mr. Spencer that 'the Second Law of Motion is an immediate *corollary of the preconception* of the exact quantitative elation between cause and effect.'"

Now let the words italicised be compared. In the first passage Mr. Spencer is said to hold that the Second Law of Motion is a preconception. In the second he is represented as maintaining that it is a *corollary* from a preconception. Is not this "confusing issues"?

Mr. Hayward has the choice of two alternatives. He may admit that one of these statements is a misrepresentation of Mr. Spencer's doctrine, as was alleged. Does he refuse to do this? Then he may transfix himself on the other horn of the dilemma, and boldly assert that in his view a preconception and a corollary from a preconception are one and the same thing. But until Mr. Hayward can arrive at some agreement with himself as to the terms in which he shall state Mr. Spencer's theory, the conclusion of impartial outsiders will probably be that he is not yet in a position to pronounce authoritatively on the merits of it.

"A Senior Wrangler" is good enough to say that my letter makes him feel "something like Alice behind the looking-glass." After this amenity, one may be pardoned for stating the position which *his* mental altitude leaves. A famous metaphysician once wrote an essay to prove that the narrow discipline of mathematics produces an incapacity for general reasoning. Sir W. Hamilton would have found his *à priori* arguments confirmed if he could have read the letter of "A Senior Wrangler."

The "Senior Wrangler" quotes a sentence of mine to the effect that "the experiences these propositions record all implicate the same consciousness—the notion of proportionality between force applied and result produced; and it is out of this latent consciousness that the axiom of the perfect quantitative equivalence of the relations between cause and effect is evolved." He does not quote a previous passage in which it is said:—

"Here, as in the examples about to be given, the relation between cause and effect, though numerically indefinite, is definite in the respect that every additional increment of cause produces an additional increment of effect; and it is out of this and similar experiences that the idea of the relation of proportionality grows and becomes organic."

It might have been supposed that the doctrine so expressed was effectually guarded against misapprehension. Are not the preconceptions derived from the child's muscular experiences described as *numerically indefinite* (*i.e.* not expressible in *proportional numbers*)? Is it not said that out of them the idea of the relation of proportionality *grows?*" In the very sentence quoted by the "Senior Wrangler," is it not said that the notion of proportionality is *implicated* in the child's consciousness, and that the physical axiom comes from this *latent* consciousness? And yet the "Senior Wrangler," looking down from his mathematical heights, and catechising me as he would a schoolboy, asks me whether I know "what proportionality means"?

But for the letter of a "Senior Wrangler," one would have believed that it was made clear to everyone that the notion of proportionality generated by these early experiences was vague and general, not exact. How else should I have said that from it "the axiom of the perfect quantitative equivalence of the relations between cause and effect is *evolved?*" After thrice reading "First Principles" does not the "Senior Wrangler" know that being evolved includes passing from indefiniteness to definiteness? How then can he pretend that it is meant that the child gets from his experiences the knowledge that a double effort produces in all cases just double the result? The argument obviously implied is that this is the *finished conception* finally arrived at by the adult, as holding in those cases where causes and effects are uncomplicated.

Having but limited space, and assuming that the requisite qualifications would be made by unbiased readers, I passed over all those details of the child's experiences which would have been required in a full exposition. Of course I was aware that in the bending of a stick the visible effect does not increase in the same ratio as the force applied; and hardly needed the "Senior Wrangler" to tell me that the resistance to a body moving through a fluid increases in a higher ratio than the velocity. It was taken for granted that he, and those who think with him, would see that out of all these experiences, in some of which the causes and effects are simple, and in others of which they are complex, there grows the consciousness that the proportionality is the more distinct the simpler the antecedents and consequents. This is part of the preconception which the physicist brings with him and acts upon. Perhaps it is within the "Senior Wrangler's" knowledge of physical exploration, that when the physicist finds a result not bearing that ratio to its assigned cause which the two were ascertained in other cases to have, he immediately assumes the presence of some perturbing cause or causes, which modify the ratio. There is, in fact, no physical determination made by any experimenter which does not assume, as an *à priori* necessity, that there cannot be a deviation from proportion without the presence of such additional cause.

Returning to the general issue, perhaps the "Senior Wrangler" will pay some respect to the judgment of one who was a Senior Wrangler too, and a great deal more—who was distinguished not only as a mathematician but as an astronomer, a physicist, and also as an inquirer into the methods of Science: I mean Sir John Herschel. In his "Discourse on the Study of Natural Philosophy," he says:—

"When we would lay down general rules for finding and facilitating our search, among a great mass of assembled facts, for their common cause, we must have regard to the characters of that relation which we intend by cause and effect."

Of these "characters" he sets down the third and fourth in the following terms:—

"Increase or diminution of the effect, with the increased or diminished intensity of the cause, in cases which admit of increase and diminution."

"Proportionality of the effect to its cause in all cases of *direct unimpeded* action."

Observe that, in Sir J. Herschel's view, these are "characters" of the relation of cause and effect to be accepted as "general rules for *guiding* and facilitating our search" among physical phenomena—truths that must be taken for granted *before* the search, not truths derived *from* the search. Clearly, the "proportionality of the effect to its cause in all cases of direct and unimpeded action" is here taken as *à priori.* Sir J. Herschel would, therefore, have asserted, with Mr. Spencer, that the Second Law of Motion is *à priori;* since this is one of the cases of the "proportionality of the effect to its cause."

And now let the "Senior Wrangler" do what Sir J. Herschel

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